ON THE

RELATIVE TEMPERATURE

OF

ARTERIAL AND VENOUS BLOOD.

BY

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This great question may appear at first sight to be a simple one, but an examination of its history will certainly lead to a very different conclusion. The nature of the inquiry is to some extent revealed in the contrary answers given by those who have investigated the subject. While some have decided that arterial blood is the warmer of the two, others are in favour of venous blood; and, again, others have asserted that no difference exists, the temperatures of the two being equal.

Now, whence this discrepancy? If we assume the relative temperature of venous and arterial blood to be fixed and invariable, it is very obvious that, while one class of observers is right, the other two must be wrong. I believe the discordant results of the various experiments which have been from time to time recorded to be due to the mode in which the investigation was conducted.

It must be confessed that all have hitherto failed to convince others of the truth of the conclusions founded upon the several experiments, for the question is still generally re-

garded as a doubtful one.* But, whereas some of these experiments may be considered merely as inconclusive or unsatisfactory †, I believe others can be shown to be obnoxious to fatal objections.

As an illustration of the former class, I will select the experiments of Dr. Davy, for none are better known or so often referred to. Of those upon the blood in the heart he says: —

"In each instance the animal was killed by the division of the great vessels of the neck; an opening was made immediately into the thorax, and a very delicate thermometer was introduced into the ventricles of the heart by means of a small incision. The operation occupied so short a space of time that in three instances the right auricle had not ceased contracting."‡

Now the objections to such experiments as these are rather negative than positive. Inasmuch as they were performed upon dead animals, in which the circulation and respiration had ceased, they are obviously exposed to so many sources of error that the evidence they furnish cannot be regarded as conclusive or satisfactory. It must be admitted that they still leave the question an open one.

But to those experiments which have resulted in declaring venous blood to be warmer than arterial, I believe this fatal objection will, with scarcely an exception, apply. The circulation has been so obstructed by the means employed, that the natural changes, which the blood undergoes in the lungs, have been considerably impeded or altogether arrested; and, consequently, a state approaching more or less nearly to what is termed asphyxia, has been induced.

* It is worthy of notice that in the *last* edition of his work on Anatomy, Dr. Sharpey omits all allusion to this question. This silence is very significant.

† On this subject see especially Georg. v. Liebig, Ueber die Temperaturunterschiede des Venosen und Arteriellen Blutes, Giessen, 1853, who enters into an extended criticism of previous experiments.

‡ Researches, Physiological and Anatomical. By John Davy, M D., F.R.S. 1839. Vol. i. pp. 138–9.

Of those physiologists who have investigated the question, some have tested the comparative temperature of the blood in the arteries and veins; some, the blood in the two ventricles of the heart; others, and these constitute the majority, have employed both methods.

Now it is to be observed, that although experiments upon the blood in the vessels have generally agreed in fixing the temperature of arterial above venous blood*, experiments upon the blood in the heart itself have, on the contrary, yielded such apparently discordant results. It is in these experiments upon the heart that the pulmonary circulation has been obstructed.

And it is not difficult to perceive the effect which obstruction of the pulmonary circulation must have upon the question at issue. If the temperature of the blood in the left ventricle be higher than that in the right, the increase must be produced in the course of the pulmonary circulation; consequently, whatever interferes with the respiration must interfere with the change which is consequent upon it. This will be presently demonstrated.

In illustration of this statement, I will take the most recent of these experiments. Amongst other methods of ascertaining the relative temperature of arterial and venous blood, G. Liebig practised the following †:—

"A dog was killed, and a ligature tied tightly round the neck, to prevent the lungs from collapsing. In this way, as the pulsations of the heart lasted a short time longer, some of the blood would be arterialised by the air in the lungs, and more or less arterialised blood would therefore always be found in the left side of the heart. The thorax was then opened by as small an opening as possible, the aorta tied at its bend, and the vena cava superior close to the auricle; then, the dog being raised, and the ligatures drawn upwards,

* Including the beautiful ones of Becquerel and Breschet with the thermo-multiplier.

† Op. cit., pp. 10, 11. British and Foreign Medico-Chirurgical Review, vol. xii., 1853, p. 253.

in order to make the opening in the vessels the highest point, thermometers were passed into the two ventricles, down the artery and vein. After the introduction of the thermometers, cotton-wool was placed in the opening of the thorax, in order to prevent any real difference between the temperature of the blood occurring from the difference in the thickness of the two ventricles."

In the first place, all the objections which apply to observations of this nature after death, hold good against this experiment.

Moreover, the dog is not only killed, but immediately before death a ligature (stocking or handkerchief) is tied tightly around the neck, thereby producing the very condition which should be most anxiously avoided—a state of asphyxia. "In this way, as the pulsations of the heart lasted a short time longer, *some* of the blood would be arterialised by the air in the lungs, and *more or less* arterialised blood would therefore always be found in the left side of the heart." Some! Unless the natural condition of the pulmonary circulation is maintained, and the normal changes are effected in the blood as it flows through the lungs, the experiment is a failure. Dr. Davy was aware of this, for he says*,—"In death by asphyxia there is generally an accumulation of blood in the right ventricle; and in many instances I have observed, when the right ventricle has been distended with blood, little difference of temperature between the two sides of the heart;" and in a note he adds,—"In instances of death by asphyxia, theoretically considered, it might be expected that the blood, returning from the lungs to the left side of the heart, not having been acted on by the air, and there having been no production of heat, would necessarily resemble venous blood, in temperature as well as in colour."

G. Liebig says the use of the handkerchief was to prevent the blood from being cooled in the lungs, by preventing the

* Op. cit., p. 149.

change of air. But in this way all natural changes whatever are prevented.

I will endeavour to show hereafter that this and other similar experiments afford evidence of the very opposite nature to the conclusions which are drawn from them. No wonder that, "in the experiments on the recently-dead animal, the temperature of the blood in the right ventricle was found once higher, and twice equal, to that of the left." That is, the temperature of the blood in the left ventricle fell, in comparison with the blood in the right, according as the asphyxia which had been induced was more or less complete.*

I repeat — the same or similar objections will apply to all the earlier experiments on the same side.

Still more recently C. Bernard has investigated this subject very elaborately, and with his usual skill and ability.† Bernard ascertained the temperature of the blood in the right and left ventricles by the following method: — A dog being conveniently fixed upon the back, he made an incision in the lower part of the right side of the neck, and isolated successively the right jugular vein and the carotid artery as low down as possible without penetrating into the chest. An incision was then made in the artery and vein, and a thermometer introduced into the left ventricle and into the right, and around it a ligature was placed upon the vessel, moderately tight, simply for the purpose of preventing the entrance of air into the right heart and the escape of blood from the left. "It is known when the left ventricle is entered, because the thermometer cannot advance further, and because it is shaken by the contractions of the heart. It is easy to enter the right ventricle down the jugular vein and through the auricle. For the left ventricle it is neces-

* The results of G. Liebig's experiments upon the blood in the vessels of living animals form exceptions to the general rule. According to them, venous blood is constantly warmer than arterial.

† Comptes Rendus, 1856, Septembre 15. Recherches Expérimentales sur la Température Animale.

sary, upon dogs, to enter by the carotid, than by the brachio-cephalic trunk, and afterwards the aorta, to enter directly by it into the left ventricle. It happens sometimes that one tears, in this operation, the sigmoid valves of the aorta, and this accident will cause some disturbance of the circulation." To this he says he shall refer upon some future occasion, "when we study the modifications which the disturbances in the circulation effect upon the temperature of the blood. For the present we will only report upon the results obtained upon animals which were calm, and in which the conditions of the circulation were as normal as possible." He adds: — "In my experiments I have always used a single thermometer, which I introduced successively into the right ventricle and into the left, repeatedly changing it, to verify by numerous observations my facts."

The experiments were subsequently repeated upon sheep. These animals having a very long neck, the origin of the vessels from the heart may be almost reached without opening the chest; consequently the operation can be accomplished with great facility. In them, also, comparative observations were made with the same thermometer, introduced successively and alternately into the right and left ventricle.

In these experiments, Bernard found that the blood in the right side of the heart is warmer than that in the left, and concludes, therefore, "that the change of venous into arterial blood in the living animal does not coincide with an increase, but on the contrary with a diminution, of temperature."

Moreover, from previous experiments *, Bernard declares that the excess of temperature in the right ventricle is due to the blood which enters it from the liver by the inferior vena cava, for in the superior vena cava the blood is cooler.

It appears to me, that a moment's reflection upon the function of the cardiac and arterial valves will suffice to con-

* Comptes Rendus, 1856, Août 18. From which he concludes that the blood in the hepatic veins is warmer than elsewhere, and that the liver is the focus of animal heat.

vinee us of the effect upon the circulation which must attend the operation just described. When we consider their office, and the effect upon their action of introducing the thermometer in the manner described, I imagine we shall not be inclined to admit that in any such experiments the animals were calm, and "the conditions of the circulation as normal as possible." But it is needless to pursue this argument. I refer to a future experiment in proof of the validity of my objection.

Now, having endeavoured to show that previous experiments are open to certain fallacies which vitiate their results, or at all events to objections which militate against their conclusions, I propose to relate some of those which I have performed. How far they are imperfect or erroneous, I leave to others to determine.

The purpose of Experiments 1 and 2 is to ascertain the relative temperature of arterial and venous blood in its natural and healthy condition. The first is upon the blood in the heart, and the second upon the blood in the vessels.

Experiment 1.—In a dog under the influence of chloroform, I exposed the heart by removing a portion of the anterior wall of the chest immediately over it. During this operation, the left pleura was wounded, and the lung partially collapsed, but the respiratory movements were not apparently affected. The heart was acting vigorously and regularly. I first punctured the left ventricle with a trocar, and immediately introduced a thermometer. It rose to between 99° and 100°. In the same way, I placed another in the right ventricle, and it rose to between 97° and 98°. The shock occasioned by the use of the trocar disordered the heart's movements for a few seconds, but it soon recovered, and resumed its natural action. The thermometers remained stationary for some time. Then, as the respiratory movements became feebler, the right ventricle gradually became distended, and the thermometer in it rose to nearly 99°. Almost simultaneously that in the left ventricle began to

fall, so that for a short time the two thermometers exactly corresponded. The left continued slowly to sink, and soon afterwards the right began to fall also, and so the temperature of both gradually decreased until I removed them. The heart was still feebly acting when the thermometers were removed, but the respiratory movements had entirely ceased. The application of chloroform was discontinued before the thermometers were introduced.

P. M. — Both lungs were healthy, and crepitant throughout, very little collapsed, and remarkably free from congestion. Each ventricle of the heart was filled with a clot of dark blood.

In this, and in all my experiments, I previously tested the thermometers I employed, and ascertained that they accurately corresponded.

Experiment 2. — In a large dog rendered insensible by chloroform, the abdominal cavity was opened, and a thermometer was passed through the left renal vein into the vena cava immediately below the liver: it rose to 96°. Another thermometer was then passed through the arteria sacra media into the aorta immediately above its bifurcation: it rose to 97°. In neither case was the flow of blood through the vessel arrested. During the period the thermometers were kept in the vessels, and for some time after, chloroform was occasionally discontinued. The circulation and respiration proceeded naturally. Neither vessel was exposed more than was necessary, and the vein the less of the two. Both thermometers maintained their respective levels; perhaps that in the vein rose more slowly to its maximum than the one in the artery.

A small aperture was then made (for another purpose) through the walls of the chest on the right side, between the fifth and sixth ribs, through which air rushed. The respiratory movements still continued.

The heart was now exposed by removing the anterior wall of the thorax in front of it. In doing this the left pleural cavity was opened, and the lung collapsed. After-

wards no further respiratory movements were observed beyond some convulsive gasps. The heart was much distended, but still vigorously acting. A thermometer introduced into the right ventricle stood at 95°; another placed in the left stood at 95° also. When the left ventricle was punctured, it was observed that the blood which escaped was as dark as that in the right. The heart continued to act for some time after the experiments were concluded.

Both lungs when examined were found collapsed.

This dog had not been fed for thirty hours previously, and the last meal had been a scanty one.*

These experiments need no comment; they speak for themselves. The diminution of the blood's temperature, when observed in the heart, and the equality of the two sides in the latter part of the second experiment, is explained by the fact that, although the heart was still acting, the respiration had previously ceased. See the fourth experiment.

G. Liebig objects to the removal of a portion of the anterior wall of the thorax; for, he says†, by exposing the heart in this way the blood is unequally cooled in the two ventricles. The wall of the right ventricle being thinner than that of the left, the blood contained therein loses its temperature more rapidly. In proof of this, he quotes experiments to show that when a thermometer is placed in each ventricle of the exposed heart in a dead animal, the one in the right falls more rapidly than the other in the left; that when a heart is cut out and immersed in water, a thermometer placed in the right ventricle indicates change of temperature more rapidly than one in the left; and that, lastly, when a heart is excised and filled with water, the right side cools more quickly than the left.

This objection is "specious, but not satisfactory." The experiments which he advances fail to support it, inasmuch as no circulation is going on. This constitutes an essential

* See future remarks, p. 20.

† Op. cit., pp. 12, 13, 14.

distinction. In his experiments fluid is stagnant in the ventricles; in the living animal there is a constant change. Experiments of this nature practised upon dead animals are certainly obnoxious to this objection; but that it cannot be extended to the case of a living animal in which the circulation continues is, I think, proved by the following experiment: —

Experiment 3. — In a room, the temperature of which had been raised above 80° (it was as warm as we could conveniently bear it), a large dog, which had not been fed for more than twelve hours previously, was placed under chloroform. As small a quantity was administered as was compatible with apparent insensibility to pain. I immediately exposed the heart, with as little violence as was possible, and placed one thermometer in the right and then another in the left ventricle. I introduced them by first puncturing the walls of the ventricles with a small double-edged knife: a few drops only of blood escaped. In the right the thermometer rose to 102°, and in the left to 102¾°. The left pleura was slightly wounded in exposing the heart, but the animal still continued to respire and the heart to contract. For ten minutes the thermometers remained unaltered, indicating the same temperature as at first; then the left began to fall very slowly, and when it had fallen ½°, the right still maintained its original level. The fall of the left side followed almost immediately upon the cessation of the heart's action. After death, each ventricle contained a moderate amount of blood: they were not distended. The blood, both arterial and venous, was of the ordinary colour and appearance.

In this experiment, which is also a confirmation of the previous ones, the fact is sufficiently conclusive, that, after the introduction of the thermometers, the temperature remained stationary for ten minutes, and that then the first symptom of decline was observed in the left. The details of the first experiment afford evidence to the same effect.

From some experiments which I have performed to illus-

trate the effect which a state of asphyxia has upon the relative temperature of arterial and venous blood, I select as an example the following: —

Experiment 4. — A dog, rendered insensible by chloroform, was strangled by a cord drawn tightly round the neck. The heart was immediately exposed by the removal of a portion of the anterior wall of the thorax. A faint tremulous action was still visible. The right ventricle was distended; it was punctured, and a thermometer was introduced. It rose to 101½°. The puncture and the escape of a small quantity of blood were followed by much more decided and regular contractions of the ventricles. Another thermometer, introduced in a similar manner into the left ventricle, rose to 101°. The blood which escaped from this ventricle was venous in character. In the course of one or two minutes both began to fall, but the right was rather in advance. Temperature of room, 60°.

Here, as in the experiments by Liebig and others, by preventing the change of the blood from venous to arterial in the lungs, the difference of temperature naturally existing in the ventricles is actually reversed. The loss of temperature observed on the left side in this experiment would doubtless have been more considerable if the means adopted to produce asphyxia had not checked to some extent the evaporation of fluid, and the consequent consumption of heat from the blood at the lungs, which occurs when the air is constantly renewed.

The fifth experiment was designed to show more fully the relation which exists between the natural change which the blood undergoes in the lungs and the elevation of its temperature. It shows that, when deprived of the action of oxygen, notwithstanding the pulmonary circulation continues and its other conditions are fulfilled, there is a loss instead of an increase of temperature.

Experiment 5. — A large dog, having been rendered insensible by chloroform, one thermometer was introduced through the arteria sacra media into the abdominal aorta at

its bifurcation, and another into the inferior vena cava from the right common iliac vein, at about the same level. The circulation through the vessels still continued. The intestines were partially replaced, so as to cover the vessels. After a few moments, when the effects of chloroform were subsiding, the one in the artery stood at 98°, and the one in the vein at 97°.

The trachea was then rapidly exposed, and a large tube, with a stopcock, was secured in it through which the animal respired freely, the thermometers maintaining their original temperatures. A bag, filled with pure hydrogen, was then fitted to the tube in the trachea, and the animal was made to respire that gas. Almost immediately, the thermometer in the artery began to fall; and, after a few seconds, both thermometers stood at the same level—96°. Simultaneously, the blood was observed to grow darker.

The heart was then rapidly exposed. As the natural respiratory movements were failing, artificial respiration, by alternate compression and relaxation of the bag, was very easily kept up. The blood, which issued from several arteries which were divided in exposing the heart, was observed to be quite dark. The heart was still acting regularly, but not vigorously: it was not at all distended with blood; on the contrary, it was rather flaccid. A thermometer was then placed in each ventricle; the one in the left stood at 96°, and that in the right at 96½°. The blood in both ventricles was quite dark. The lungs were pink; perhaps rather darker than natural, but not perceptibly congested. They expanded and contracted alternately with the action of the bag fairly and very freely. When pricked, gas immediately issued from them, and they collapsed.

The sixth experiment is after the method adopted by Bernard. It illustrates the effect produced upon the temperature of the blood by disturbing the circulation in another way.

Experiment 6. — In a large dog, under the influence of chloroform, I exposed the carotid artery and jugular vein at the lowest part of the neck, on the right side. Having

punctured the artery, I passed a long probe down it into the left ventricle, and secured it by a ligature. In the same manner I passed a second probe down the vein into the right ventricle. Then, while the probes remained as I had placed them, I exposed the heart by removing a portion of the anterior wall of the thorax, as in my previous experiments. Both ventricles were very much distended with blood: they were acting vigorously, but evidently labouring against the obstacle which the probes caused to their free action. A thermometer introduced into the left ventricle, stood at 102½°; another introduced into the right, stood at 103¼°. After the lapse of a few minutes, the thermometers in both ventricles began to fall. The temperature of the room during the experiment was somewhat under 60°.

One word with regard to the use of chloroform in these experiments. Its employment appears to me to cancel one great objection to such proceedings. The severe shock and disturbance which such extensive mutilations must otherwise necessarily produce will always, I think, tend to invalidate, to a greater or less extent, the conclusion. Vivisections without chloroform are always more or less objectionable, and especially those on the circulation, for it is not possible to inflict pain without disturbing it; but under its influence the animal is passive and insensible, and the severest measures produce no apparent effect.

But the use of chloroform may be objected to in these experiments on other grounds, inasmuch as it has been shown*, that when animals are fully under its influence their temperature falls, and this effect has been supposed to be due to its direct action on the blood, which in some instances has been observed to become darkened.† I have

* By Dumeril and Demarquay. Comptes Rendus, 1848, vol. xxvi. p. 171.

† Dr. Jackson examined the body of a woman who had been killed by chloroform. The blood had lost the power of coagulating, and of becoming red by exposure to the oxygen of the air.—Comptes Rendus, Fév. 25, 1856.

See also Chassaignac, The Lancet, Feb. 21, 1857.

M. Gruby, on the contrary, concludes from certain experiments that the

anticipated this objection, and in answer to it I will simply state at present that, from numerous experiments, I have ascertained that the depression of temperature consequent upon the employment of chloroform is not an immediate effect, but only ensues after a considerable interval, varying of course with the dose, but always exceeding that during which its action was kept up in any of my experiments.*

In none of these experiments was chloroform observed to produce the slightest effect upon the character of the blood.

Moreover, if any error were due to chloroform in these experiments, it must be on the right side; for chloroform must chiefly act, whether directly or indirectly in this respect, by diminishing the changes which the blood naturally undergoes in the lungs.

In truth, this last observation may be more widely extended. All errors connected with any disorder or disturbance of the circulation and respiration which these experiments involve, must be on the right side. The rise in the blood's temperature, which is consequent upon the changes effected in it at the lungs, must of course vary as the conditions of the pulmonary circulation are fulfilled, and decline in proportion as those changes are diminished or in any way interrupted. For instance, the accidental puncture of the left pleura in exposing the heart, which occurred in some of the experiments, by preventing the full and complete expansion of the lung, could only act in this manner by diminishing to a corresponding extent the increase of temperature with which the blood returned to the left ventricle. In proof and illustration of this I refer to the second experiment.

It is right to mention that these experiments have not

vapour of chloroform, so far from changing arterial into venous blood, increases the intensity of the red colour of arterial blood, and changes also dark blood in the veins to bright blood. — Comptes Rendus, 1848, vol. xxvi. p. 175.

Dr. Snow, London Medical Gazette, 1851.

Certainly neither of these contradictory effects can be regarded as simply due to the ordinary action of chloroform.

* See also the experiments of Dumeril and Demarquay, op. cit.

been performed very many times. I have repeated them only to satisfy myself and those who witnessed them of their accuracy.

Now, if these experiments be true, will they not assist us to reconcile the apparently discordant results of those which have been previously referred to? Whilst the first, second, third, and former portion of the fifth show that, so long as the natural pulmonary circulation continues, the blood returns from the lungs to the left side of the heart warmer than when it was sent there from the right side, and that, consequently, arterial is warmer than venous blood; the fourth, fifth, sixth, and latter portion of the second, illustrate and confirm the statement, that in proportion as those changes are impeded or arrested, so will their natural result be correspondingly diminished or altogether destroyed.

The experiments of Bernard, Liebig, and others of a like kind, bear similar testimony. Viewed in this light, therefore, they do not contradict, but rather confirm, the fact which I have endeavoured to establish. They assist to prove that the relation between the change which occurs in the blood at the lungs and the elevation of its temperature is one of cause and effect.*

There can be no doubt that the question whether arterial or venous blood is the warmer, is one of primary importance, for it must lie at the very foundation of any correct theory of animal heat. It is impossible to reconcile the fact that arterial is warmer than venous blood with the most current theory of the production of heat—that which removes its seat from the pulmonary, and places it exclusively in the systemic capillaries. The admission of the fact that venous is cooler than arterial blood, turns one's mind from the modern theory in the direction of the older, and now almost forgotten one. But although incompatible with the assumption that the only change which the blood undergoes in its

* So that it is not saying too much to affirm, that in any given instance we can determine by the means we adopt whether the blood on the left or on the right side of the heart shall be warmer.

temperature during its circulation through the lungs is a loss due to evaporation, it is by no means at variance with the belief that heat is generated during its circulation through the systemic capillaries also.

The doctrine most completely in harmony with ascertained facts is that which refers the source of heat (1.) to the action of oxygen inspired upon the blood as it circulates, and (2.) to those changes between the blood and the tissues which are involved in nutrition.

That blood returning from the systemic capillaries is cooler than when it entered them, only proves that more heat is there removed from the blood than is produced in it by the chemical changes connected with nutrition. If it were not for these, and doubtless the changes still going on in the blood itself also, supplying, to a great extent, the demand for heat, the blood would be much more cooled during its passage than it is.

The following experiment will probably illustrate what is meant: it was practised on an arm a short period after death: —

Having secured a stopcock directed downwards in the brachial artery just above the elbow, I connected it, by means of an elastic tube, with a reversible syringe. In the tube immediately above the stopcock, I placed the bulb of a thermometer, and another in the median basilic vein. By immersion in warm water, the temperature of the limb having been raised to 100°, I injected water of 100°, as indicated by the thermometer in the tube, through the arm, until it escaped freely from apertures in the superficial veins in front of the elbow. After the injection had been continued very rapidly for some time, the thermometer in the vein varied from 85° to 90°, thus indicating a loss of from 10° to 15° during its circulation through the arm. After some time, the water returned less freely, and the limb became tense and distended.

Again: having passed a thermometer through the arteria sacra media into the aorta of a dead dog, and another into the inferior vena cava through either the renal or common

iliac vein, and then having raised the body by immersion in warm water to the temperature of the living animal, I have rapidly injected from above water varying from 100° to 110° through the posterior portion of the body, allowing it to escape freely by the vena cava. After a short time, there was always a difference between the two thermometers of 10° or more.

This experiment is advanced as an illustration only. It is far too rough and imperfect to prove anything; but, so far as it goes, it shows how much heat may be abstracted from the circulating fluid by the tissues when no compensating production of it ensues. Such an experiment, if more skilfully repeated, would probably be attended with less loss, but always, beyond doubt, with much more than naturally occurs.

In relation to this subject, the fact is worthy of notice that, notwithstanding some apparent instances, it has never yet been satisfactorily shown that the temperature of any part, when unduly exalted, ever exceeds, under any circumstances, that of the blood itself. With regard to the local increase of temperature which occurs after injury to a portion of the nervous system*, Brown-Séquard concludes that "the degree of temperature of paralysed parts depends on the quantity of blood they receive, and this quantity varies according to the size of the arteries and capillaries of these parts."†

When we consider the character and composition of our food, and its ultimate destination, it seems impossible to resist the conviction that a very large proportion of animal heat is produced in the blood itself by the direct combination of oxygen with certain of its constituents. It may be very difficult, if not impossible, to determine what proportion the amount of heat so produced bears to that which is generated

* On Local Increase of Temperature following Section of Nerves, see Bernard, Comptes Rendus, Août 18, 1856.

Also Brown-Séquard. Experimental Researches applied to Physiology and Pathology, 1853.

† Op. cit., p. 77.

by the changes which occur between the blood and the tissues; but, as an important element in the calculation, we must consider the proportion which the directly combustible part of our food bears to the plastic. "It appears that in milk and in the different varieties of corn (which are the most perfect forms of nutritive matter) the proportion which the plastic bears to the respiratory materials, are 1 of the former to from 3 to 6 of the latter."*

Under these circumstances, it is hardly possible but that the amount of heat furnished by these two sources must be constantly varying, dependent as it is in great measure upon external circumstances. Assimilation†, the quantity and quality of the food consumed, and therefore of the materials introduced into the blood, the activity of the changes proceeding in the tissues, and other causes constantly operating, must influence the result. Therefore it is possible that some future investigations may detect a greater difference in the relative temperature of arterial and venous blood in herbivora than in carnivora.

But if it be admitted that heat is produced in the blood itself by the action of oxygen upon it, where is such action so likely to occur as in the pulmonary capillaries, where they first meet, and where the conditions for union are so favourable?

Nor are facts wanting which directly prove that there is an immediate union of this kind in the blood at the lungs. Such, for instance, is the one recently discovered, that the sugar formed in the liver may be traced into the pulmonary artery, but is not found in the pulmonary veins. At the lungs it disappears from the blood.

It was objected to the old theories which referred the source of heat exclusively to the lungs, that, if it were so, their temperature would be very considerably above what it

* Elements of Chemistry, by W. A. Miller, M.D., V.P.R.S., Part iii. p. 740. 1857. In woman's milk, according to Liebig, the proportion of the plastic to the respiratory food is as 10 to 40.

† Bernard concludes, from certain experiments, that after a full meal the difference of temperature between venous and arterial blood is diminished, but that the absolute temperature of both is raised. Op. cit.

is.* The present doctrine cannot be considered at all objectionable in this respect, when the comparatively slight difference which exists between arterial and venous blood, the absolute temperature of the lungs themselves†, and the extensive evaporation which is constantly going on there, are borne in mind.

A similar remark will apply to another objection to the same theories, that $C O_2$ is still for some time given off from the lungs when no O is admitted to the blood. The present doctrine is quite compatible with the results of the experiments. The production of $C O_2$ would not be immediately arrested by the deprivation of O. The amount produced would be considerably diminished, but it might be found so long as the blood contained any O. These experiments, however, require repetition.

Our knowledge concerning the amount of the different gases contained in arterial and venous blood is at present too inaccurate and uncertain to enable us to found any conclusion upon it. What is known, so far as it goes, accords perfectly with the doctrine that there is a direct action between the blood and oxygen at the lungs. There can be no doubt that O and $C O_2$ are contained in both arterial and venous blood, and that O exists in a larger proportion in the former. The evidence concerning $C O_2$ is less conclusive. Information on this subject is chiefly derived from the experiments of Magnus; but even these, with respect to the latter point, have been differently interpreted by different authors.‡

Lastly, it is only fair to mention the experiments of Dr. Davy in relation to this subject. Dr. Davy conceived that he had demonstrated the production of heat by the action of oxygen on the blood. A very thin vial was selected, and carefully enveloped in bad conducting substances. To this

* This objection was certainly fatal to Lavoisier's theory. It applied with less force to that of Sir H. Davy.

† Berger found the temperature of the brain 104·5, of the liver 106·3, and of the lungs 106·5 in the sheep.

‡ See Quain and Sharpey's Anatomy, 1856, p. 54. Dr. Carpenter in his several works on Physiology. Handbook of Physiology, by W. S. Kirkes, M.D., 1856. p. 178. &c.

a perforated cork was adapted, holding a delicate thermometer. Two or three cubic inches of mercury were then introduced, and immediately after it was filled with venous blood, kept liquid by agitation. "The vial was now corked and shaken; the thermometer included was stationary at 45°. After five minutes that it was so stationary, the thermometer was withdrawn, the vial closed by another cork was transferred inverted to a mercurial bath, and one and a half cubic inches of oxygen were introduced. The common cork was retained, and the vial was well agitated for about a minute. The thermometer was then introduced; it rose immediately to 46°, and, continuing the agitation, it rose further to 46·5°, very nearly to 47°." In two other experiments, the details of which were slightly varied, there was an increase of 1°, and in one on arterial blood a rise of $\frac{1}{2}$°.*

Now, if these experiments were valid, they would prove a good deal, perhaps too much.

I have repeatedly performed the experiment of agitating venous blood with different gases. These experiments were generally conducted in the following manner, after the method adopted by Davy: —

Two twelve-ounce bottles were each provided with an accurately-fitting glass stopper, and a cork, through the centre of which a thermometer passed, reaching to within a fourth of the bottom. A similar quantity of mercury, varying from six to sixteen ounces, having been placed in each bottle, they were both rapidly filled with venous blood drawn from the jugular vein of a calf, and immediately stoppered. The blood always retained its venous appearance. In many instances, before abstracting the blood, the trachea was compressed so as to render it still darker. During coagulation, which proceeded very slowly, the bottles were occasionally shaken, to break up the clot. After a time, when the blood had attained a settled temperature, by means of a mercurial bath, a certain quantity of oxygen was introduced into one bottle, and a similar quantity of hydrogen, or carbonic acid

* Op. cit., vol. ii. p. 168.

gas, or common air, into the other.* Each was closed with the cork to which the thermometer was adjusted, and the temperature observed. Both were then shaken for the same period, being usually at the same time enveloped in flannel, and the temperature was again noted. They were then generally allowed to rest for a few minutes, and the temperature again observed. Sometimes the agitation was repeated twice or thrice.†

The details of these experiments were often varied, more especially with regard to the relative proportion of blood and gas, and the size of the bottles.

Similar experiments were performed with arterial blood.

It would be tedious to record at length the whole of these experiments, and perhaps unfair to select only a portion of them. The results were not constantly uniform, but often variable, owing no doubt to slight variations in the details of different experiments. The general conclusions I drew from them were these: —

1. That when venous blood was treated in this manner with oxygen, its temperature was usually raised from 1° to $1\frac{1}{2}$° or 2°.

2. That when venous blood was treated in a similar manner with hydrogen or carbonic acid, its temperature was as frequently raised, and generally to the same extent.

3. That similar experiments upon arterial blood usually yielded the same results.

4. That, in all cases, the increase of temperature seemed to be the result of the agitation. By shaking water in a similar manner with air, a small quantity of mercury being present, I have often raised its temperature, though to a less extent.

I have ascertained from other experiments that the increase of temperature is not imparted by the hand or from any external source.

* The difficulty of obtaining nitrogen absolutely pure was the reason why this gas was not employed.

† In these and other experiments Mr. Barford kindly assisted me. I have very often availed myself of his skill in practical chemistry.

It is to be borne in mind that the character of the evidence which these experiments furnish, does not depend so much upon their absolute accuracy as upon their comparative results. Two experiments at least were always performed at the same time, and under similar circumstances, with different gases. While the temperature was almost always raised in both instances, no excess was observed in the case of oxygen.

It appears, then, from these, that we must distinguish between the facts and the conclusions in Dr. Davy's experiments. The temperature of blood treated with oxygen in the manner described does, as he states, rise; but it also rises when similarly treated with other gases. Even water, when shaken with common air under the same circumstances, rises too. It cannot, therefore, be admitted that the increase of temperature in Dr. Davy's experiments is due to the action of oxygen on the blood.

It is not to be wondered at that a repetition of these experiments should lead to a negative result, although they may appear upon the first view to be satisfactory and conclusive. If living circulating blood, exposed to the action of oxygen under the most favourable circumstances, is only warmed to so trifling an extent, is it reasonable to expect a similar result from the imperfect action of oxygen upon dead coagulated blood?* In these experiments, as in many others, Nature is too coarsely and clumsily caricatured.

At present there is no evidence upon which we can safely venture further into this inquiry. If, as I conclude from my experiments, arterial blood is warmer than venous, the increase of temperature must occur in the lungs as a result of those changes which the blood there undergoes. Of the nature of those changes little or nothing is known.

* See some recent experiments by Dr. Harley in relation to the action of oxygen upon blood. Proceedings of the Royal Society, 1856, p. 78.

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